short lengths, and attaches platina rings to each end. The rings are hooked upon a hooked fork, and the whole is plunged into heated nitric acid, when the silver coating is dissolved.

The artist may now wire his cell according to his fancy. Mr. Ulrich's plan seems to be, to hold one end by an overplate; then to allow the wire to be stretched by its platina ring, and to fix the other overplate. He recommends using a cell of the same material as the wire, as, otherwise, a difference of expansion might break or slacken the wires.

On the properties of Rock as a foundation of the Piers of Meridian Instruments, with an Account of the Detection of a hitherto unsuspected Cause of Error in the Edinburgh Transit. By Professor C. P. Smyth.

Some years ago, doubts were expressed of the fitness of a rock foundation for an observatory. It does not appear that any experiments were made, or that any reason was adduced beyond this, that as tremor was unfavourable to the performance of large telescopes, and as rock was more capable of transmitting tremors than less compact material, therefore rock was to be avoided when choosing a site for an observatory. The author or authors of this opinion were probably but ill acquainted with the mode of working an observatory, or the requisites for obtaining accuracy in meridian observations; yet it is certain that an undue importance was attached in some cases to these very idle surmises. At the present time it is not likely that any intelligent person would be misled by such authorities, and it is therefore unnecessary to mention here the mischief they have caused.* It is to be wished that the founders of future observatories, who can command a rock foundation, should make use of their good fortune; and that those who cannot, would look carefully to the possible effects of moisture, which are probably more extensive, and vary more rapidly, than those of temperature.

* The effect of tremor on a telescope is probably familiar to every reader of this notice. It causes a sort of burr round the object, and destroys the sharpness of outline and definition. This is probably more injurious in reflecting than in refracting telescopes; but we may fairly doubt whether it is more felt on solid than on loose foundations. In a *standard* observatory, where observations are made principally in the meridian, tremor scarcely affects the *accuracy* of observation at all, unless it is so excessive as to change the position of the microscopes, piers, &c. Now this is obviously the least likely to happen when the foundation is on rock; the tremors are propagated through the substance, without in any respect altering its form. Sudden and discontinuous changes, which obey no law, are those only which are to be feared in a well-directed observatory. Tremor is chiefly objectionable as disturbing the mercurial horizon, which, however, is now mostly used as a verification, not as the ordinary mode of observing; and when this inconvenience only occurs occasionally, it can generally be avoided or palliated by a little contrivance or foresight. Unless the *adjustments* are kept in a fluctuating and uncertain state by occasional small oscillations (and we believe no careful experiments have been directed to this point), they are minor evils. The experience of the Oxford and of the Edinburgh Observatory is, so far as it goes, conclusive against any danger from moderate exposure to tremors in a well-founded and well-managed observatory.—*S.*

The observatory of Edinburgh is placed on the Calton Hill. This is chiefly of a porphyritic formation. The apex was blasted away to obtain a level area, on which the observatory was erected. The site of each pier was cut away until a sound part of the rock was arrived at (it was not necessary to go deeper for this purpose than six or nine inches), when the exact size of the foundation was at once marked out and the space carefully levelled. The foundation stone was also carefully smoothed, and then laid in its place with milk of lime. As the foundation and stone were both rather hollow, except for three inches at the outer edge, which was polished, the fitting was very perfect. There are no vertical joints, and each stone was laid in the same manner as the foundation stone. As one of the principal thoroughfares of Edinburgh runs about one hundred feet below, and only three hundred feet distant from, the observatory, tremors were confidently predicted by the alarmists. Professor Henderson, however, found none, nor any interruption to his observations in mercury. Professor Smyth adds that he finds no annoyance from the railroad about three hundred feet below, and at a horizontal distance of five hundred feet.

So far the observatory founded on a rock came out victoriously from its ordeal, but Professor Henderson, in the course of his work, found a well-marked annual variation of the *level* of the transit, which he attributed to the expansion of the rock. This variation seemed so intimately connected with temperature that he latterly took his factor for level correction from the thermometer, having found a constant agreement between this and the indications of the spirit level. The maximum of this change amounted to between $0^{s}\cdot 2$ and $0^{s}\cdot 3$ in the value of the level factor, and the variations were tolerably regular.

On computing the *azimuthal* factors for 1841, Professor Smyth was very much disturbed on finding variations, which sometimes altered the factor as much as $0^{s}\cdot 3$ in a day, and more than $1^{s}\cdot 0$ in the course of the year. These changes in azimuth had been remarked by Professor Henderson, and were attributed to the irregular action of the counterpoises, which were consequently removed. On a comparison of these errors with the indications of thermometers plunged in the rock there were apparent marks of correspondence.

There are several thermometers inserted at different depths in the rock near the observatory, which had been carefully observed in the year 1841.* The indications of these thermometers were projected on paper, and the curves thus formed compared with a curve traced according to the course of the azimuthal deviation. It was thus made evident, that the curve of azimuthal deviation, though having, like the other curves, an annual maximum, did not otherwise resemble the curves belonging to the deep-seated thermometers at all; and, in fact, it came nearest the curve traced out by the

* Some years ago, Professor J. D. Forbes had four thermometers sunk in the rock with their bulbs at the depths of 24, 12, 6, 3 French feet and a fifth on the surface merely covered with sand.

thermometer attached to the barometer and by the free thermometer exposed to the outer air. Hence the cause of the deviation was not to be looked for in the effect of temperature on the foundations or on the massive transit piers, but on smaller parts more readily affected, such as the metallic mounting. These were accordingly examined. In the azimuthal Y, the construction was found to be much as usual, but the artist has adopted an adjustment for the vertical Y, which seems liable to suspicion. There are two vertical screws applied from below; one, pushing, on the north side of the middle, and the other, pulling, at the south side. The Y is prevented from turning in a vertical plane by jamming horizontal screws, which press a plate against the north face of the Y so as to bring the whole tightly against a stopping-piece, which blocks the south face. Professor Smyth's present opinion is, that the effect of expansion on the two screws, which are in contrary states of constraint, is to alter the adjustment; certainly the arrangement looks unmechanical. In the ordinary mode of construction, in this country at least, the elevating Y is either raised by one central screw, or by two screws, one on each side of the centre; in which case a drawing-screw may be placed at the centre. There is thus no tendency to twist, and the side-plates which confine the Y laterally have to exert little restraining force. Professor Smyth has communicated with MM. Repsold, the makers of this magnificent instrument, and is awaiting their reply before adopting any remedy.*

Extract of a Letter from Mr. Lassell.

On July 8, about $1^h\ 45^m$ A.M., the planet *Neptune* was seen with three stars in the field of the micrometer: *a*, a star of the 10th magnitude; *b*, a star of the 12th; and *c*, smaller still, but indisputably visible. This *c* is probably a satellite. The star *a* precedes *b* about $11^s{\cdot}2$, and is to the south of *b*. The angle which the line joining *a* and *b* makes with the parallel of daily motion is $32^\circ{\cdot}35$. *Neptune* was to the south of *b*, and somewhat following. The distance by estimation from *b* was 3 diameters, and the line joining *a* and *Neptune* made an angle of $28^\circ{\cdot}7$ with the parallel. The supposed satellite was as nearly as possible in the parallel of *b*, and rather farther than *b* from the planet. Thus its position with respect to *b* and the planet was very well defined.

On the morning of July 9, Mr. Lassell again set his telescope on the planet, which now was very nearly in a line with *a* and *b*, though nearer *b*; but he could see no trace of *c*. The atmosphere was not quite so favourable as on the preceding occasion, but his impression

* Sudden and lawless changes in azimuth forbid independent determinations of the azimuthal deviation (which are also the best), viz. from the consecutive semidiurnal transits of circumpolar stars. The possessor of an imperfectly mounted instrument must content himself with assuming the fundamental places of his close circumpolar stars, and determine his azimuthal error from each of them. This will, with proper caution, be found quite sufficient for objects not too near the pole, especially when the clock-error stars are pretty numerous, and situated above and below the object to be determined.